

Elpidia glacialis, has been dredged at this inconsiderable depth, and even this was dredged in the Arctic Ocean, where true abyssal forms are to be met with at comparatively shallow depths. This species too can exist at immense depths, one from Station 160 having been dredged at a depth of 2600 fathoms, the greatest depth at which any Holothuroid has to this been dredged being 2900 fathoms. Among the more remarkable and distinguishing characteristics of this order Mr. Théel mentions the agreement in several important details—both in their internal anatomy and outer forms—of the adult and larval forms, an agreement more close than occurs in any previously known Holothuroid. He does not agree with Danielssen and Korren in placing the *Elasipods* low in the series of the Holothuroids; nay in some respects he regards them as having attained to a higher development than all the other Echinoderms, because, among other facts, their bodies are distinctly bilaterally symmetrical, with the dorsal and ventral surfaces distinct and often with a cephalic region well marked. Only the ventral ambulacræ are subservient to locomotion; these latter show a tendency to appear both definite as to place and number. The dorsal appendages are so modified as to perform functions different from the ventral ones. This memoir contains forty-six plates, which give full details of the forms and structure of all the new species.

LIGHT

Light: A Course of Experimental Optics chiefly with the Lantern. By Lewis Wright. (London: Macmillan and Co., 1882.)

THIS is a book by a worker whose work in his own line is of a very high order, and whose experience will be of correspondingly high value to others who are working at the same subject. In all those departments of experimental optics in which the lantern is employed for the demonstration of actual experiments to an audience, Mr. Wright is a master hand: and his book, as might be expected, is consequently a valuable repertory of useful information and of suggestive hints. Of books on Light there are already enough and to spare. Of standard treatises and text-books in the department of Geometrical Optics the supply is more than could be desired. In Physical Optics there is still room for a good elementary mathematical text-book. In Physiological Optics also there is, save for the great treatise of Helmholtz, a void. But the work before us stands apart from all these, both in aim and in character. Indeed so well does it carry out the ideal of a work "on experimental optics chiefly with the lantern," that there was really no need to prefix to the title the word "Light." True it is that Mr. Wright does not confine himself to the mere working of lanterns and their accessories. He deals in a simple and practical way with the laws of reflexion and refraction, and with ordinary optical instruments: but he always adds something of practical interest to the teacher of optics. To illustrate the laws of reflexion and refraction he describes a simplified form of the apparatus so well known in Prof. Tyndall's lectures on Light; and the mechanical illustrations of wave-motion, &c., are also new in several respects. The chapter on Spectrum Analysis is brief and sketchy, but includes almost all the

experiments which can be projected on to the screen with the lantern. Amongst these we notice very careful instructions for exhibiting the spectrum of Newton's rings and of other interference phenomena.

Nearly one-half of the book is devoted, and well devoted, to experimental work on Double-Refraction and Polarisation. In this section there are a number of beautiful experiments described which we do not remember having seen before in any treatise in the English language. Amongst these are some with compound mica plates built up of a series of films of definite thickness and united by Canada balsam. A series of twenty-four superposed mica films, each producing a retardation of one-eighth of a wave-length and each one-sixteenth of an inch shorter than the one beneath it, is in this way made to reproduce exactly the first three orders of colours of Newton's rings, but divided into the precise tints over narrow strips. A detailed account is also given of the combinations devised by Norremberg and Reusch for reproducing the phenomena of uniaxial crystals and of quartz by the superposition of thin films of mica crossed in various ways. Plates illustrative of these combinations contribute much to the value of the descriptions and explanations of the text. Mr. Wright also gives some account of his own researches upon the spiral figures produced by the introduction of quarter-undulation plates into the polariscope in which crystal sections are being examined by convergent light. There is a penultimate chapter on the polarisation of the sky and of minute particles, followed by a final chapter—wholly out of place in such a work—in which, so far as it is intelligible, there appears to be an attempt made to connect the undulatory theory of light with the trinitarian theory of theology. With the exception of this last, and with a few occasional inelegancies of style, there is little fault to find with the book. The mathematical student of optics will without doubt grumble when he takes up the work, because the mathematical aspect of the subject is conspicuous by its absence. The author does not profess to be a mathematician: or he would hardly have pronounced in favour of Brewster's views on the theoretical polarising angle, as he does on p. 223. This, however, is a minor matter in a book whose great aim is to assist manipulation. The numerous illustrations, a large proportion of which are original, add greatly to its value. The coloured plates of polariscopic phenomena are, it should be added, of singular excellence.

S. P. T.

OUR BOOK SHELF

Practical Chemistry, Analytical Tables, &c. By J. Campbell Brown, D.Sc. (London: Churchill, 1882.)

NOTHING perhaps is more remarkable than the great increase during the past few years in the number of books on practical chemistry and analysis. This has no doubt to some extent been caused by the prominence given generally to the teaching of chemistry in the laboratory.

The books to which we refer consist with few exceptions of tabular statements of reactions of acids and bases and methods of detection of the same in simple salts or mixtures. They all appear to be on the same "type" and with the same intention of putting students through a course of drudgery in qualitative analysis according to a fixed "table." The book before us is no worse than others of its class, but attempts rather too much by giving

condensed tables for alkaloids and gases, which are, however, in themselves very good ones. It is to be feared that these practical books tend to make students mere analytical machines in a small way, without giving them much real practical notion of chemistry. It is questionable whether a student who has worked through the modern tabular system of practical chemistry would be able, for instance, to state the reason for the employment of bricks in preference to chalk for the back of an ordinary fireplace or some equally simple practical question.

Elementary Chemical Arithmetic. By Sidney Lupton. (London: Macmillan and Co., 1882.)

THIS little book with its modest preface will be recognised by all teachers of chemistry, especially in large laboratory classes, and also by students as a really useful adjunct.

Unfortunately in large public laboratories a considerable proportion of the students have been very much neglected in the matter of their elementary mathematical education, or it has been of such a nature that they are not able to apply it to the solution of ordinary chemical problems, thus entailing, in many cases, a large amount of extra work and loss of time on the part of the teacher in giving instruction in elementary arithmetic. This book fits into its place exactly. It is divided into two main portions: an introduction, consisting of short but very understandable explanations of arithmetical processes in common demand in chemistry and physical chemistry of a practical and elementary nature, the second portion being problems divided under the headings of the different elements. Regarding these it may perhaps be said that they do not err on the side of being too chemical, and in one or two cases more attention has been given to the question as a question than to its absolute chemical correctness, but these are mere details that in no way detract from the utility of the book for its purpose.

What is required of the mass of chemical students is that they should be able to apply methods of reasoning founded on experimental facts in the science to the solution of concrete and abstract problems; and working through this book will certainly conduce to bring about an improvement in that direction.

The Watch and Clockmaker's Handbook. By F. J. Britten. (London: Kent and Co., 1881.)

THIS little book has been written, we are informed, chiefly for the instruction of country watchmakers. It cannot fail to be agreeable to them: it contains a great deal of useful practical information, and some is given of a higher quality, such as workmen are, to their credit, eager for now-a-days. To another and wider circle there is also much of a character to be interesting. The book is a proper supplement to the more popular horological treatises. There are good descriptions and pleasing diagrams of the various watch escapements; there is a chapter upon the art of springing; the mechanism of chronographs, repeating watches, and calendars is shown, but almost too briefly. Lastly, we find pictures and a short reference to the various tools which watchmakers employ, and some serviceable memoranda are added. Upon the whole the author has and deserves our praise.

H. DENT GARDNER

Heroes of Science. Botanists, Zoologists, and Geologists. By Prof. P. Martin Duncan, F.R.S., F.L.S. (London: The Society for Promoting Christian Knowledge, 1882.)

THIS little volume contains brief sketches of the lives of a few botanists, zoologists, and geologists, for the most part acknowledged compilations from well-known sources. No doubt the work will serve the purpose for which it is evidently intended—that of interesting young people in science.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Physics of the Earth's Crust

ON March 23 last Prof. Green sent to NATURE some remarks upon Mr. Hill's review of my "Physics of the Earth's Crust." More lately the third edition of his "Physical Geology" has appeared, in which he has repeated the substance of a part of what he then wrote. On account of the great weight which his authority will carry, I think I should offer some reply.

He truly says at p. 674, that I claim to have proved that the contraction of the earth through cooling cannot have caused the amount of squeezing and elevation which has taken place, and that the hypothesis is therefore insufficient to explain the facts which it professes to account for; but he then adds: "What Mr. Fisher has really done is this. His calculations go far to prove that, provided the earth cooled in the way assumed by Sir Wm. Thomson, contraction would not suffice to produce anything like the compression and elevation that has actually occurred. But this is quite another thing from disproving the contraction hypothesis. Mr. Fisher's investigations tend rather to establish a strong probability that the earth did not cool in the way supposed by Sir Wm. Thomson,"—that is, that it became solid throughout in a comparatively short space of time. But of course my calculations do not establish any probability against this way of cooling, unless we begin by assuming that contraction through cooling has been the cause of the elevations. And that seems to be begging the question. What they do prove is that the contraction hypothesis will not account for the elevations if the earth has cooled as a solid.

But there may have been another way of cooling which, on geological grounds, I believe to have been the true one. The earth may not have become solid throughout in a short space of time, and may not be solid even now. In that case the crust, whose corrugations we have to account for, must have floated on a denser liquid substratum. Under these circumstances every elevation above the mean level must have had a corresponding protuberance answering to it below. This is necessary, as was long ago pointed out by Sir G. B. Airy. I have, then, proved that, this being so, if the crust beneath the ocean is of the same density as beneath the continents, on what I conceive to be reasonable assumptions regarding the thickness and density of the crust and the density of the substratum, a shortening of the earth's radius by less than 700 miles would not have sufficed to produce the existing inequalities. I can imagine no theory of the constitution of the interior that would admit of so large an amount of contraction taking place, after the whole had become sufficiently cool for a crust to have begun to be formed, as to cause such an amount of shortening as this.

If, however, we suppose that the crust beneath the oceans is denser than that which forms the continents (and I have given several reasons for believing such to be the case), then a much smaller amount of radial shortening would suffice. I have estimated it at about forty-two miles. Still, anything near this shortening is far beyond what any reasonable amount of contraction from cooling could produce. For if there be a liquid substratum this must be of nearly equable temperature throughout, and that cannot be much above the temperature of solidification; so that it does not appear how a much greater contraction can be got out of the gradual solidification, and incorporation of the upper parts of the liquid layer with the crust, than could be obtained on the former supposition of a cooling solid globe; and I have shown that, in that case, the radial shortening would be less than two miles.

Thus, then, I claim to have disproved the contraction-hypothesis under the two alternative hypotheses (1) of a solid globe, and (2) of a liquid substratum.

Capt. Dutton, of the United States Geological Survey, has said of this part of my work, "First and foremost he has rendered most effectual service in utterly destroying the hypothesis, which attributes the deformations of the strata and earth's crust to interior contraction by secular cooling. No person, it seems to me, can sufficiently master the cardinal points of his